We would like to thank the Referees for their valuable comments and critics that we tried to take into account in the revised version of the manuscript. Hopefully, all the major and minor corrections pointed out by the reviewers have been corrected now. A detailed answer follows below. We provide replies to the reviewer' comments in bold. Since most of the corrections suggested by the referees consisted of minor corrections to the text, all of them have been accepted and included in the new version of the text. Only those corrections that imply some discussion are commented below.

Answers to Referee 1

The authors have partially addressed my concerns. I still believe the paper has interesting results that merit publication, but I do not think it is ready yet.

1) I still can not follow the reasoning behind the mechanism that links the summer FTLE and rainfall in winter. The authors say "In our opinion, large-scale tropospheric mixing drives summer SST anomalies that lead to changes in the next seasons storm tracks, and consequently changes in the location of action centers". However, it is not possible to deduce this from their results.
They show composites of different fields during summer for large + and - cases of FTLE anomalies. However, there is no causality implied there. SST lags atmospheric forcing for about 3 months, and thus the simultaneous maps shown can not be used to imply causality. Also, it is not obvious that midlatitude SST forces atmospheric circulation anomalies during winter that lead to rainfall.
An alternate possibility is that the FTLE in summer is a reflection of tropical SST anomalies, which may persist and then impact winter precipitation. Note that both winter rainfall and FTLE are correlated to SOI in JFM.

Several previous studies have related the influence of North Atlantic SSTs on precipitation in different European areas, namely, in Sardinia, Italy (Delitala et al., 2000); in southwest England (Phillips and McGregor, 2002); in Iceland (Phillips and Thorpe, 2006); in Iberian Peninsula and Northern Africa (Rodriguez-Fonseca et al., 2006; Lorenzo et al., 2010). In all of them, a delay of several months for a maximum of correlation has been observed.

In this paper, we open a new possibility to understand this correlation. Our hypothesis is that summer FTLEs (large-scale tropospheric mixing occurring during summer) activate the anomalies of SST in the Atlantic Ocean modifying the next months mid-latitude atmospheric circulation. Changes in the circulation will lead to more or less precipitation in the west of the Iberian Peninsula.

The second possibility raised by the referee concerning the link between tropical SST anomalies and summer FTLEs could also be an explanation to the observed precipitation correlation. However, our studies suggest that the observed correlation between summer FTLES and the next winter SOI is only significant at 90%. We did not calculate the influence of winter SOI on next summer FTLEs.

Although out of the scope of this paper, the use of an atmospheric model to simulate the influence of atmospheric mixing onto the SST and its latter precipitation increase could be of great help.

References

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- Delitala AMS, Cesari D, Chessa PA, Ward MN. 2000. Precipitation over Sardinia (Italy) during the 1946–1993 rainy seasons and associated large scale climate variations. International Journal of Climatology **20**: 519–541.

- Lorenzo MN, Iglesias I,Taboada JJ, Gómez-Gesteira M. 2010. Links between circulation weather types and teleconnection patterns and their influence on precipitation patterns in Galicia (NW Spain). International Journal of Climatology **30**: 980–990.
- Phillips ID, McGregor GR. 2002. The relationship between monthly and seasonal south-west England rainfall anomalies and concurrent North Atlantic sea surface temperatures. *International Journal of Climatology* **22**: 197–217.
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2) When describing Figure 3 (page 5) the authors propose a series of changes in synoptic activity without providing evidence for that. If they believe this is the right explanation they should at least say that this is an hypothesis, or speculation.

We agree with the referee and we changed the explanation of Fig.3.

3) In the response the authors mention that
"the changes in the values of positive phase and negative phase of FTLEs are small, the Wilcoxon rank sum test shows that these differences between positive and negative phase of FTLEs are significant. The same is found between the differences of the positive and negative anomalies of the other variables."
If this is certainly the case, they should explain in the manuscript the use of this test and mark in the figures the regions that are really significant. This is important given that the changes in FTLE are of the order of 3%.

The Wilcoxon rank sum test is a nonparametric test for two populations.

Figure 3 shows the areas (pointed) in panels (a) and (b) where the differences between both FTLE phases are really significant, obtained from a two-sided Wilcoxon rank sum test. This test has been applied to both phases of the FTLE anomalies time series for each location shown in panels (a) and (b).

On the other hand, NOAA only provides spatial maps for the composites of anomalies of the other climatic variables, obtaining in all cases that both phases are significantly different. In this case we cannot show in which positions are significant or not. Otherwise, we should download the full climatic variables data sets and calculate the anomalies directly from the time series repeating the same procedure done for the FTLE.

Answers to Referee 2

The authors have addressed some of the recommendations from my original review to improve the presentation of their work. However, I feel that some further revision is necessary before this work can be considered ready for publication.

My main concern is still the definition of seasons, which is non-standard. Despite the statements in the response letter, there is a climatological standard referring to DJF (JJA) as (boreal) winter (summer). If a climatologist reads the term "(boreal) summer/winter", they will most likely expect exactly these months to be considered. I accept that some authors may deviate from this general practice for probably good reason, but in this case, I strongly recommend clarifying throughout the manuscript that the three-month periods of JFM and JAS are used and NOT the common climatological winter and summer months. That is, I recommend replacing the terms "summer" and "winter" by JAS and JFM, respectively, throughout the manuscript to avoid any ambiguities.

To avoid ambiguities we have tried to replace or include the acronyms JAS and JFM along the text.

The second, yet minor point of criticism is a number of sentences where rephrasing is necessary for either scientific or grammatical reasons. I provide a list of these points below (page and line numbers refer to the final manuscript markup without tracked changes).

We really want to acknowledge the referee for his/her detailed list of corrections that we are sure will improve the paper. All of them have been considered in the new version of the text. However, some of them are commented below.

• Page 3, ll. 9-10: This sentence appears a bit misplaced at this point; better provide this information at the point where the correlations are discussed.

The sentence has been removed from Section 2.1. and parts of it included when Fig.2 is described.

• Page 4, ll. 10-14: Please detail the meaning of the terms "mean" and "total mean" (i.e., "mean" seems to denote the spatial mean at a given time step, and total mean the average FTLE over all time steps?). Also, the phrase "two time series (positive and negative phases)" is not clearly understandable as such.

Total mean refers to the climatological period 1981-2010 used for the anomaly plots by the Earth System Laboratory (NOAA). While *mean* corresponds to the mean of years with positive/negative (above/below the median) summer FTLE. Then, two time series (positive and negative phases) of these seasonal composites were calculated for years with positive/negative summer FTLE anomalies. We have corrected the last paragraph in Section 2.2 to better describe these terms.

• Table 1: Please clarify if the seasons (rows) correspond to the same year as "winter"/"summer" (should be "JFM"/"JAS") or the preceding/following.

We agree with the referee that it was not clear the timing of the seasons. To avoid that, we have included the symbols +1, 0 and -1 to indicate that the correlation is with the next, same or previous year season. Thus, for example, summer (JAS) FTLE correlate with next year AMJ NAO with a coefficient equal to 0.34, and winter (JFM) rainfall correlates with previous year OND SCA with a coefficient equal to 0.36.

• Fig. 3, caption: What do you mean by "global summer FTLE mean" – surely not the summer FTLE mean around the globe. Moreover, when you select years with positive/negative FTLE

anomalies – do you mean all years where the mean FTLE is above/below its global mean, or just such where this deviation is particularly strong (as one would commonly use when considering “climatological composites”)?

We agree with the referee that this sentence was not clear. We have deleted the word “global” as it has no meaning at all in this context. The meaning of summer FTLE mean has been explained at the end of Section 2.2 in agreement with your question answered above.

Answers to Referee 3

Following the recommendations of the reviewer we have rephrased the paragraphs or added some sentences as suggested. All of these corrections have been included in the new version of the text. However, some of them are commented below.

First of all, I think that include some references in several parts of the text can improve the quality of the manuscript. I suggest include references on the next points:
*** Page 1: When the authors write: "However, the relationship between NAO and ENSO and the European variability is nonstationary; that is, the strength of the correlation between these two teleconnections and climate anomalies has changed over time."**

These references are already in the paper al the end of the next sentence where we discussed the non-stationary of these two modes. (Vicente-Serrano and López-Moreno, 2008; Rodríguez-Fonseca et al., 2016).

*** Page 2: On the affirmation: "Changes in mid-latitude circulation can strongly affect the weather events", I suggest to add a reference and also a region where this changes can be observed, referring the type of weather events that are affected.**

We added the suggestion of the referee in the paper;

- Screen, J. A., and I. Simmonds, Amplified mid-latitude planetary waves favour particular regional weather extremes. Nat. Climate Change **4**, 704–709, 2014.
- Marshall, J. et al. North Atlantic climate variability: phenomena, impacts and mechanisms. Int. J. Climatol. **21**, 1863–1898, 2001.

*** Page 2 and 3: Please include a reference or web for the IB02, ERSST, ICOADS, NCEP reanalysis and CPC (related with teleconnection indices) databases.**

We have added new references and a new Table with the web sites.

Other comments and suggestions are the next ones:

Page 2:
The authors include some works that link the Iberian precipitation with other variables. The paragraph is the next one:
"Previous works have shown a possible link between the Iberian precipitation and other variables like summer Sea Surface Temperature (SST) anomalies over the north Atlantic basin (Rodríguez-Fonseca and deCastro, 2002; Lorenzo et al., 2010; Hatzaki et al., 2015), other teleconnection patterns (deCastro et al., 2006; Casanueva et al., 2014) or the Euroasian snow cover in autumn (Brands et al., 2014). The storm track activity has been related to the occurrence of extreme events (Lehmann and Coumou, 2015). Changes in mid-latitude circulation can strongly affect the weather events."
Please, can you clarify how the Iberian precipitation can be linked with the Euroasian snow cover in autumn, and also where the storm track activity was considered and where the occurrence of extreme events was analysed?

The main storm-track activity occurs in mid-latitudes. The paper by Brands et al. (2014) describes the correlation among the Iberian precipitation and the Euroasian snow cover in autumn.

I also suggest to include, in a short way, the main methods used to calculate the FTLE in the next paragraph: "Our goal in this study is to characterize the rainfall patterns in the Iberian Peninsula as a function of the large-scale tropospheric mixing over the Atlantic ocean. To that end, we have calculated a climatology of FTLE for the period 1979-2008. The obtained

time series was then correlated with the precipitation over the Iberian Peninsula. Finally, we discuss the obtained results by considering their relationship to the main modes of circulation variability."

The sentence "*To that end, we have calculated a climatology of FTLE using finite-difference approximation to the deformation gradient for the period 1979-2008.*" has been included.

Page 2 and 3:

The authors said that they considered "Yearly anomalies of the SST, geopotential at 500 hPa, sea level pressure (SLP) and wind speed at 200 hPa and 850 hPa for the same period have been used. The SST anomalies have been derived from The Extended Reconstructed Sea Surface Temperature (ERSST) dataset which is a global monthly sea surface temperature dataset derived from the International Comprehensive Ocean-Atmosphere Dataset (ICOADS). It has been derived on a 2 x 2 grid with spatial completeness enhanced using statistical methods. This monthly analysis begins in January 1854 continuing nowadays. The newest version of ERSST, version 4, is based on optimally tuned parameters using the latest datasets and improved analysis methods. The geopotential, SLP and wind speed anomalies have been obtained from the National Center of Environmental Prediction (NCEP) reanalysis with a spatial resolution of 2.5 x 2.5. The climatology used for the anomaly plots is for the 1981-2010 period." (page 2 and 3). Nevertheless, in the page 4 they said that "Seasonal composites (averages) of the anomalies (mean - total mean) of the SST, geopotential height and wind speed were obtained from NCEP for the same period." Please, can you clarify which kind of data is used and with which temporality? Also the authors said that the performed a climatology for the anomaly plots using the 1981-2010 period. Why this period was selected and not the 1979-2008 period that is the one chosen for the entirely work?

We agree with the reviewer that the period selection could be confused so we added at the end of section 2.1 the following sentence;

The climatology used for the anomaly plots is for the 1981-2010 period. This period is considered to be the standard period for climatological studies of anomalies.

Please, considered to place the sentence: "The monthly indices were averaged for the seasons JFM, AMJ, JAS and OND from 1979 to 2008. The most representative atmospheric patterns for the Northern Hemisphere were considered in order to analyse their influence on precipitation for the Iberian Peninsula and with the summer FTLEs." at the beginning of its paragraph on this way: "The monthly indices were averaged for the seasons JFM, AMJ, JAS and OND from 1979 to 2008. The most representative atmospheric patterns for the Northern Hemisphere were considered in order to analyze their influence on precipitation for the Iberian Peninsula and with the summer FTLEs. The teleconnection indices NAO, SCA (Scandinavia pattern), EA (East Atlantic pattern), EA/WR (East Atlantic/ West Russia pattern), POL (The Polar/ Eurasia pattern), SOI (Southern Oscillation Index), PNA (Pacific-North American Pattern) and the atmospheric mode AO (Artic Oscillation) were obtained from the Climate Prediction Center (CPC) at NCEP at monthly time scales."

We consider that it was correctly written as monthly indices refer to the teleconnection indices previously described.

Why the authors considered an initial separation of 0.35º in the distribution of the particles?

The FTLE measure the deformation of an approximation to a continuum material surface. The model resolution is 0.7ºx0.7º, so at least the minimum resolution considered should be 0.7º since all

points should be forced by the wind field at starting times in order to have a smooth deformation field. To that end, we have considered a four times higher resolution to solve this item without compromising so much the computational cost.

Page 4:
It will be interesting to comment that the FTLE were initially calculated globally, and then an area of the Eastern Atlantic was extracted to perform the study. Please, considered to include a square in the Figure 1 that represent the Eastern Atlantic area used to extract the FTLE results (between 30ºW and 0ºW and between 25ºN and 65ºN).

Included.